portion of the brick-earth in which the implements were found at Botany Bay, near Thetford, in Suffolk. The announcement at once provoked strenuous opposition, and therefore, on a tour of inspection of Mr. Skertchly's work with Mr. Bristow, we took care to examine into this point. The result was that I satisfied myself of the truth of Mr. Skertchly's observations that the implement bearing brick-earth in places underlies a boulder-clay, which, in his opinion, is not of the earliest date, in which case the men who made these tools must have been of inter-glacial age."

The "strenuous opposition" to which Prof. Ramsay refers, was directed, it will be remembered, not against the possibility of human remains being found under glacial deposits, but against such a conclusion being accepted without the clearest and most irrefragable evidence being adduced in its support. And it must be borne in mind that a number of most competent observers have examined the sections in question, and have arrived at conclusions directly opposed to those announced by the officers of the Geological Survey. When, therefore, our author, still speaking of this question of the contemporaneity of man with the glacial epoch, goes on to exclaim: "Perhaps we cannot prove it, but there is nothing improbable in the hypothesis, and I am not the only one who believes it," we cannot help entertaining the feeling that this is hardly the spirit in which a scientific question should be treated, and that the method which he adopts is one scarcely calculated to carry conviction to the mind of any competent judge of the matter.

In laying down this book we cannot refrain from once more expressing our opinion that it is a work of the highest value, and one worthy to take a foremost place among popular manuals of science. The illustrations are excellent; the woodcuts, by Mr. Sharman—giving a very faithful representation of species which have been selected by Mr. Etheridge as characteristic of the several formations—are quite new, and some views of scenery have also been added to those contained in former editions of the book. The little geological map of Great Britain, which we are glad to see reproduced, is a marvel of clear and accurate printing in colours, and well sustains the reputation of the publishing firm which has produced it.

FLORAL DIAGRAMS

Blüthendiagramme. Construirt und erläutert von Dr. A. W. Eichler, Professor der Botanik an der Universität Kiel. (Leipzig: W. Engelmann. Theil i., 1876, Theil ii., 1878.)

THIS book supplies a want that every real student of systematic botany must have felt. The introductory chapters are devoted to an inquiry into the morphology of the flower and its parts, and the inflorescence; while the subsequent chapters are a full exposition under the head of each family and order of the floral type and its most important modifications. Preceding each order is a list of the most important works bearing on it, and every quotation is accompanied by a full reference. Hence the book is both a Thesaurus of the literature of its subject, and moreover a Prodromus of phanerogamic morphology. Despite the modest title, the vegetative arrangements are explained wherever they present interest, and the same ungrudging pains are often extended to fruit and seed. Unlike too many authors Prof. Eichler is utterly free from provincialism. He cites as freely and constantly foreign botanists as those who have used the German language. Unfortunately we are but poorly represented, for morphological research is all but unknown in England, and is untaught by both our swarms of botanical lecturers and the great institutions which are the outward and visible sign of what Government recognises as botany. The medical curriculum has overborne original teaching by the former, the herbarium has stunted all else in the latter. Hence few of our botanists are able, like an Eichler or a Baillon, to check observations on the adult flower, with its parts distorted by drying and soaking, by their own knowledge of the growth of the living plant. Even the greatest sagacity and experience must be at a loss sometimes from this weakness in the very foundations of their work. For this reason one regrets the more that Eichler makes not a single reference to the works of Griffith, perhaps the greatest botanical genius England ever possessed, who found out for himself the value of developmental research and worked out many a flower by its aid.

A word on the method of Eichler. The actual editor of the "Flora Braziliensis," he adds to his thorough knowledge of morphology proper a rare acquaintance with systematic botany. Hence he belongs to no school, though awake to the value of workers in more limited fields, in all of which he himself has done good service. A firm evolutionist, he accepts the testimony of systematist, anatomist, teratologist, organogenist, and histogenist, and believes that all of these can in turn shed light on doubtful points. Hence his opinion must be respectfully considered by those who differ from it, and it is worth while to note a few of his conclusions.

He regards the nature of the "calyx tube" as varying with the order; truly receptacular in Rosaceæ, for instance, it is, partly at least, appendicular in some cases. The petals of Primulaceæ are regarded as true petals, and not as appendages of the stamens, a view which our descendants will have forgotten or unearth with the lazy amusement with which we look on some of the naif theories of our ancestors. The nature of the placenta and ovule is a more difficult question, and our author, who, in the preliminary chapters of Part i., published in 1876, held it essentially variable, has been led chiefly by Celanowsky's arguments to regard it as in all cases an outgrowth from the carpellary leaves. Similarly, the ovule, regarded in the First Part as a bud, is now viewed as an emergence. Of course the last word is not yet said on these points, but it is worth noting that Warming also avows his final conversion by Celakowsky, in his brilliant paper on the ovule in the first volume of the *Annales des Sciences Naturelles* for 1877; and Eichler is at one with Warming in adopting Brown's view of the female flower of Gymnosperms. It is much to be regretted that this point was not really discussed at the late congress in Paris, or that its principal advocates do not answer the latest arguments in its favour. But the question cannot at all be regarded as settled.

The cup of *Euphorbia* is regarded as an inflorescence; but though the *pros* and *cons* are fairly stated, no new light is shed on the matter.

© 1878 Nature Publishing Group

Enough has been said to show the extreme value of the book to the scientific botanist. May its teachings quicken sound study in England. MARCUS M. HARTOG

OUR BOOK SHELF

Manuel du Voyageur. Par D. Kaltbrunner, Membre de la Société de Géographie de Genève. (Zurich, J. Wurster und C^ie^, Editeurs. Paris, C. Rheinwald und C^ie^, 1879.)

A GREAT difficulty of writing a treatise for the use of travellers, on "What to Observe," lies in the impossibility of presenting to the imagination an ideal average traveller to address. If the great mass of intending travellers had much the same amount of scientific knowledge and were well grounded in the elements of science generally, a very useful and compact work might, no doubt, be composed. But as a matter of fact such persons are usually very ignorant, or variously ignorant, and a book fitted to instruct the whole of them must omit none of the more elementary considerations, and therefore would assume the shape of a collection of encyclopædic treatises. It is hard to define the level of previous knowledge to which "Kaltbrunner's Manuel du Voyageur" is best adapted. Every reader is sure to think it too diffuse for his own wants in some parts, neither deep nor full enough in a great many, and probably beyond his depth in others; but take it all in all, it is perhaps better adapted for persons of moderate culture than any similar book that could be named. It is beautifully got up, with abundant illustrations, and to say the least, would be often useful for reference and as a reminder. The range of its topics is wide enough to touch the interests of everybody, and it would be a capital present to give to a friend bound for foreign parts. Considerable space is allotted to subjects connected with social life and other anthropological questions.

There seems to be some irony in the fact that when the world is so nearly explored, manuals for the use of travellers should begin to appear. They were greatly needed many years ago, when the Admiralty Manual had the field nearly all to itself, but now that the need is less, these works are at last composed in abundance. The present one, however, is by no means intended to supply the wants of those travellers only who are exploring unknown countries, much of it being applicable even to home districts. It is less solid and more comprehensive than the recent German publication, "Anleitung, &c.," by Neumayer.

Pleasant Ways in Science. By R. A. Proctor. (London: Chatto and Windus, 1879.)

OF this book, which has been sent me for review, I can truly say that it is an excellent specimen of what has been well called (I forget by whom) *Paper Science.* A very few quotations will amply justify this verdict.

At pp. 8, 152, I find "heat" several times standing for "temperature." But the author (in these columns, vol. xvi. p. 227) insisted that

> "What *mathematicians call* the *moving force* exerted by the earth on the moon is eighty-one times greater than the corresponding force exerted by the moon on the earth."

To put "heat" for "temperature" is after all not very strange for one who puts "moving force" for "accelerating force."

In the account given of the experiments of Andrews and Tait on ozone, the action of "iodine" is given as that of "mercury": and the now-received idea of the nature of ozone—though *twice* mentioned in the paper referred to—is described as a "beautiful" and "ingeniously conceived" hypothesis suggested after the publication of the paper (pp. 351-2).

The following passage, which refers to friends of my own, I quote without comment:—

> " . . . . no one, I think, would believe so ill of his fellow-men as to suppose for one moment that *advantage could be taken of the sympathies which have been aroused by the Indian famine*, or which may from time to time be excited by the record of great disasters by sea and land, *to advocate bottomless schemes merely for purposes of personal advancement.* We must now, *perforce*, believe that those who advocate the erection of new observatories and laboratories for studying the physics of the sun have the most thorough faith in the scheme which they proffer . . . ." (p. 51).

From p. 194 I gather that I know nothing about the motion of waves, and p. 240 proves me equally ignorant of voltaic electricity. I cannot read any more.

P. G. T.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Receiving Telephones

I HAVE roughly tried two experiments which seemed likely to supply new forms of receiving telephones, and have had such partial success as seemed satisfactory in preliminary trials. As I have not time to continue the experiments I request the insertion of this note in the pages of NATURE, in the hope that some one else will follow the matter up.

In one experiment a spiral wire (I used German silver, but it may be of any material) was wound closely so that the spires were in contact, or nearly so. One end of the spiral was fixed and the other end attached by a thread to the middle of a small parchment drum-head, such as is sold by the toy-makers for thread telephones. A slight tension was put on the thread to draw the spires of wire slightly asunder. The spiral wire was then made part of a circuit, including one or two cells of Grove's battery and a line wire going to another room. By this apparatus such sounds as the scraping on a file were satisfactorily heard, although the spiral was only one coil of about an inch long. It will be observed that in this arrangement there are no magnets; the whole effect is produced by the varying induction of the current upon itself. The apparatus could obviously be rendered more efficient by using a longer spiral, or a coil consisting of a number of concentric spirals not quite in contact, so as to allow small motions to exist. The induction might be still farther increased by using a spiral of two wires, so that a powerful local current might be kept up in the alternate spires, while the varying line-current is passing through the intermediate ones. Another improvement would consist in using iron wire wound in a sufficiently loose coil. The self-magnetisation of this coil would co-operate with the electric induction to heighten the effect.

In the other arrangement an iron or steel spiral (in my experiment it was an ordinary steel spiral spring, of which the spires lay close) was placed inside a coil of copper wire in circuit with a battery and the line. The spiral, as before, was fixed at one end, and kept slightly stretched by a string connecting its other end to a drum-head. In this arrangement no current passes through the spiral, but it is the core of an electro-magnet, and becomes magnetised in a degree which changes with the alterations in the intensity of the line-current. This causes the spires to attract one another with varying intensity, and the tremulous motion so produced is propagated by the string to the parchment. By this arrangement singing, whistling, &c., were heard when a Reiss transmitter was used. Probably a soft iron coil would have been better than the steel spring I used, and the apparatus is susceptible of other obvious improvements which would add to its sensitiveness.

Before concluding permit me to thank Prof. Barrett for allowing me to make the experiments in his laboratory.

Dublin, November 25 G. JOHNSTONE STONEY

© 1878 Nature Publishing Group